DOCUMENT RESUME

ED 157 239 EC 111 216

AUTHOR Janes, Gary
TITLE School Psychological Services and Research in Schools.
INSTITUTION Iowa State Dept. of Public Instruction, Des Moines. Div. of Pupil Personnel Services.
PUB DATE Oct 77
NOTE 12p.

EDRS PRICE MF-$0.83 HC-$1.67 Plus Postage.
DESCRIPTORS *Educational Research; Elementary Secondary Education; *General Education; Research Needs; Research Utilization; *School Psychologists

ABSTRACT

The author considers the involvement of school psychologists in research projects designed to improve services and meet the educational and behavioral needs of children. Reasons why research and evaluation are important are pointed out, the kind of research needed to be done is discussed, and a brief description and schematic of a service model whereby individuals who pose practical and potentially researchable ideas and questions would be able to implement them is proposed. (SBH)

****
* Reproductions supplied by EDRS are the best that can be made *
* from the original document. *
****

ED157239

School Psychological Services

Research in the Schools

U S DEPARTMENT OF HEALTH, EDUCATION & WELFARE
NATIONAL INSTITUTE OF EDUCATION

THIS DOCUMENT HAS BEEN REPRODUCED EXACTLY AS RECEIVED FROM THE PERSON OR ORGANIZATION ORIGINATING IT. POINTS OF VIEW OR OPINIONS STATED DO NOT NECESSARILY REPRESENT OFFICIAL NATIONAL INSTITUTE OF EDUCATION POSITION OR POLICY

EC111216

Iowa Department of Public Instruction

*5100-C72791-10/77

State of Iowa
DEPARTMENT OF PUBLIC INSTRUCTION
Grimes State Office Building
Des Moines, Iowa 50319

STATE BOARD OF PUBLIC INSTRUCTION

T. J. Heronimus, President, Grundy Center
Georgia A. Sievers, Vice-President, Avoca
Robert J. Beecher, Creston
Jolly Ann Davidson, Clarinda
Ronald P. Hallock, West Des Moines
Virginia Harper, Fort Madison
Robert G. Koons, Clinton
John E. Van Der Linden, Sibley
Susan M. Wilson, Waterloo

ADMINISTRATION

Robert D. Benton, State Superintendent, and Executive Officer of the State Board of Public Instruction
David H. Bechtel, Administrative Assistant
James E. Mitchell, Deputy State Superintendent

Pupil Personnel Services Branch

Drexel D. Lange, Associate Superintendent
J. Frank Vance, Director of Special Education
Jeff Grimes, Consultant, School Psychological Services

INTRODUCTION

The purpose of this publication is to explore the concept of research as it relates to the provisions of school psychological services. The science of psychology has a research foundation and it is reasonable to consider the involvement of school psychologists in research projects designed to improve service and meeting the education and behavioral needs of children.

Appreciation is expressed to Gary Janes who contributed the material for this publication. Gary has taken a novel, creative and refreshing position in sharing some thoughts regarding school psychological services and research in the schools. Will this publication whip the reader into a frenzy which can only be satisfied by conducting research projects? Will you upon completion of this publication have a compelling need to formulate a null hypothesis? I cannot answer those questions, but I would venture this prediction: You will be exposed to some thoughts which will make you stop and think about the importance and potential of research in education and its relationship to school psychological services.

Additionally, appreciation is expressed to Diane Doolittle, secretary, for the typing of the article, and Ray Benter, graphic artist, for the cover design.

Jeff Grimes
Consultant for Psychological Services
Iowa Department of Public Instruction

October, 1977

THE SCHOOL PSYCHOLOGIST IN RESEARCH

. . . SOME THOUGHTS

by: Gary Janes Ph.D.

The School Psychologist in Research....Some Thoughts

The literature in School Psychology is filled with studies, conference reports, position statements, and graduate theses dealing with the topic of the roles and functions of School Psychologists. One customary, and by now very predictable outcome of such endeavors, has been the finding that the role of the School Psychologist as researcher gets relegated to the end of most lists of roles and functions. Under this heading, one usually finds a list of problems for which the School Psychologist's expertise could be utilized, sometimes followed by numerous claims of the virtues and necessities of this activity, along with compelling reasons why School Psychologists should engage in it, followed just as quickly be results of questionnaires, etc., which point out the reasons why, in point of fact, most School Psychologists are not likely to do it (mostly a function of the time and importance of the roles that have preceeded it in the list).

The consistency of studies in relegating research to a bottom rank of importance would seem to be evidence enough of the finality of its status that we could lay to rest the notion that it is important enough to merit consideration in its own right, quite apart from whether it takes time away from other roles. And, of course, it would be contrary to established findings to suggest that research should be "moved up" in the lists. Indeed, the continued inclusion of this bothersome term in our studies may have become rather a matter of tradition which allows us to go out way, secure in the knowledge that we are, after all, sympathetic and committed to the need for research.

Certainly, we don't need yet another study reiterating an already established fact, nor do we need to suffer the barbs of a soft voice reminding us again of the importance of research and of our sometimes silent committment to this concept, nor do we need another paper harping about this topic, do we? Perhaps not, but as can be seen by the title, since this is to be some sort of a discussion about

research, should we expect anything less from it?

Even if the answer to each of the above were yes, this paper is clearly not intended to do so, nor is it even hoped that the end result will be renewed vigor and productivity in the area of research. It is recognized that research is being done, and that some of it is being done by School Psychologists. The only aim of this paper is to present some thoughts and suggestions about an already old topic.

In fact, to prevent the disappointment of those who may feel some professional compulsion to wade through the whole sequence again in hopes of finding something unique, to lessen whatever guilt this paper may cause in us for not having engaged in such a virtuous activity, and to prevent a mass rush back to universities for further training, let me start with the following conclusions. First, all the reasons that can be found in studies for School Psychologists not doing research are accepted as legitimate and reasonable, as no rational person could logically contend that research should be done in place of other functions. Second, I can find no compelling reasons why School Psychologists should be the professionals most likely to do research than anyone else, nor even that they must at all be interested in doing research. Third, I can find no compelling reasons why all School Psychologists should be proficient in doing research. Fourth, not all School Psychologists are knowledgeable about research, and there is really no reason to believe that they should be.

To the contrary, this paper is based only on the premise that School Psychologists, like other professionals, occasionally have good ideas that could be researched, often ask themselves questions such as "I wonder what would happen if?"..."What is the best way to?"... "Why is it that?....", that these ideas and questions often are never answered because of either the lack of opportunity to pursue them or lack of knowing how to put them to test, and that although not all School Psychologists have the skills to translate these into researchable ques-

tions, some do. The problem is one of making use of the skills that some have in order to provide a service to others.

In some sense, this is bound to involve the roles of generalist and specialist. All of us may have particular areas of expertise or interest, and some will be more skillful in diagnosis, interviewing, therapy, research, etc., than others. In practice, we become specialists in particular areas that, for whatever reason, end up tickling our professional curiosity, and not necessarily because we were trained in this speciality. To whatever extent there is a body of fundamental knowledge and/or skills inherent in the profession of School Psychology, all School Psychologists should possess at least minimal levels of expertise in these areas. But one could hardly argue that in their daily practice all School Psychologists should or even could demonstrate "expert" proficiencies in all areas. The problem then becomes one of making it possible to use the expertise that does exist.

But this could hardly be a treatise on research if that was all that was said. So, for the benefit of those who are still willing to listen to an old record, and those who may still be looking for something new, let's back up and start at the beginning.

Why, in the first place, is research and evaluation so important? The list of reasons may be limited only by the imagination of the one who is proposing its importance, but among the reasons would seem to be the following: 1) Government and private agencies are increasingly making approval of grants contingent upon the evidence of good planning and sound evaluation procedures; 2) The advent of cost effectiveness accounting procedures to evaluate educational programs will increase the requirement of tangible, data based evaluation criteria; 3) The persistence of negative or inconclusive findings in the literature raises serious questions about the effectiveness of many current programs and practices; 4) The continued activity by industry in developing new educational materials, equip-

ment, programs, and tests, make necessary a selection process based on well conceived research and evaluation of such products; 5) The recent dramatic growth of special education programs and models, for which there is little evidence of their effectiveness; 6) An increasingly receptive ear to the necessity of research and evaluation seen with the advent of AEA's as service organizations to local school systems; 7) The present emphasis on legal issues and documentation which has placed the burden on education to prove the appropriateness and effectiveness of special programs and evaluation procedures.

The surprising conclusion having been reached that research is important, the next question is who should do it? It is my contention that research becomes relevant only when it originates from, builds upon, and involves the people directly affected by it. Too often, research in education is done by outsiders using the schools as a source of data. While these may eventually benefit education, the local school, area, or department needs are often neglected. The classroom teacher, counselor, speech clinician, consultant, administrator, director, and psychologist can identify problems, set goals, formulate plans, and gather data relevant to their needs. These are the people who see the kids, use the material, make the decisions, have the ideas, and ask the questions. Hence, they are in the best position to determine what research should be done. But it is unrealistic to expect that they all would do research. What is needed is some plan or system of service whereby the ideas and questions that are asked can be translated into meaningful research. School Psychologists may have specific questions and research needs unique to their profession, but they also could be of service to others by stimulating a research attitude among other school personnel, interpreting the findings of research, identifying significant problems for research, and consulting on research planning and design. As noted by the APA publication School Psychologists at Mid-Century, "research" and "service" are seen as synonymous.

One of the biggest issues among researchers may revolve around the kind of research that should be done. The issue is whether research should be basic (designed to add to an organized body of scientific knowledge and not necessarily producing results of immediate practical value) or applied (solving an immediate practical problem, with the goal of adding to scientific knowledge being secondary). The prototype of basic research is true experimental research, where one exposes one or more groups to one or more treatment conditions and compares the results to one or more control groups, all relevant variables being controlled or manipulated. The prototype of applied research is more likely to be quasi-experimental research, where one approximates the conditions of a true experiment in settings which do not allow the control and manipulation of all relevant variables.

Basic, true experimental research is recognized as the truly scientific way of building an organized theory of education. So the usual demand is for more of this and less applied research. But in this quest for true experimentation, there may be some weaknesses in the "theories" that result.

In a true experiment we attempt to randomize or control the effects of extraneous and related variables while introducing systematic variation in the experimental variable, allowing us to determine the effect of that variable. Thus, in education we attempt to ascertain the pure effect of such variables as motivation, intelligence, methods, etc., following the dictates of true experimentation, hopefully cognizant that the 'real world' effect of these variables will likely never occur in experimental form. One must wonder why we strive to study variables under such controlled conditions when it is doubtful that any variable, method, or technique has any pure effect except in the experimental sense (for how many phenomena occur in nature that are not functions of the interactions of numerous uncontrolled factors?). So we may be left with "experimental theories" which are applicable only to the degree that real life situations approximate the experimental conditions. And our theories may be scientifically encouraging, but

hardly so encouraging when the conditions are not like those under which the theory was developed.

The fact that the prospective researcher in the field will rarely find conditions amenable to true experimentation in the strictest sense, and most research will be quasi-experimental approaches aimed at the solution of practical problems. This is not to claim that there is no need for basic research, but only that we cannot refuse to research situations or phenomena that do not offer the promise of controlled true experimentation. Applied research may not lead to an organized body of scientific knowledge or to definitive theories, but if the terms "research" and "service" are to be synonymous this may be the best route. Certainly, most questions being asked in the field are not basic research questions.

A recurrent theme throughout this paper has been the need for some system of service or resource whereby individuals who pose practical and potentially researchable ideas and questions would be able to implement them, and whereby the skills that are 'out there' could be made available on an organized, systematic basis.

Following is a brief description and schematic of a service model that could function as such a resource, and that could be used to initiate and coordinate research activities throughout an AEA. The model would have two components; 1) Each service area within an AEA would have a person with assigned responsibilities in the area of research (called "research consultant" in the accompanying schematic), 2) An AEA wide research committee, possibly, but not necessarily, made up of the service area consultants.

The resource could operate in the following way: 1) a person (teacher, clinician, psychologist, consultant) has an idea or question that they would like to investigate; 2) the person with the idea contacts the research consultant in their service area for assistance in implementing the idea.

The model is seen as a resource that could be tapped by all AEA personnel,

but the emphasis would be on the model as a resource for applied individual research in which ideas or questions come from the grass roots personnel and these people maintain ownership of the project. In this sense, the research consultant or committee does not "carry out" the research, although its level of involvement would vary with the skills of the person having the idea. The research committee would function in a service or advisory capacity, as well as providing a communication network across the AEA.

As noted above, applied individual research represents the primary target of the service model, but other types of research might also be accomplished through this resource, such as evaluation of local district or AEA services and programs, and evaluative and normative studies of new test materials.